SOME QUESTIONS OF PHONETIC THEORY

CHAPTER V. The Perception of Sound.

BY

WILFRID PERRETT.

Author of "The Story of King Lear from Geoffrey of Monmouth to Shakespeare," 1904.

Reader in German in the University of London.

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PREFACE.

WHAT should have been Part II. of this work has been ready this many a moon, and awaiting publication under the title of "Peetickay: An Essay towards the Abolition of Spelling." To the five and fifty honoured patrons who have purchased Part I. I tender, together with my thanks, my apologies for the non-appearance of the promised sequel. Most of them will understand when I say that it is a question of ways and means, particularly the latter.

When in §20 I foretold a great fall for Helmholtz and his book I little suspected that the prophecy would be so soon fulfilled, by the publication of Sir Thomas Wrightson's *Inquiry into the Analytical Mechanism of the Internal Ear*, 1918. Now the case is altered. The wilderness in which I whispered to the reeds the oppressive secret, "—— hath —'s Ears", has suddenly, through a feat of invisible engineering long since planned, become populous, and no less an anatomical authority than Professor Arthur Keith has proclaimed the crudity and impossibility of the Helmholtz theory of hearing. See *The Times*, March 22, 1919, p. 9. The present chapter underlines that proclamation, bringing linguistic proof that there cannot be any resonators in the internal ear acting like "a kind of practical Fourier's theorem." The physicists (some of them) must be less superstitious.

LONDON, 31*st March*, 1919.

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conviction, and never dreaming of *camouflage* I thoroughly believed, on the supposed analogy of the digitals and dampers of the piano, that similar "damping arrangements exist in the ear, so as to quickly extinguish movements of the vibrators" (M'Kendrick). I believe my experience is not singular, for I find, in an entertaining book about noise, by a medical man, published in 1916, the statement that in the organ of Corti "what interests us specially is that it may be likened to the keyboard of a piano, and that, as Helmholtz supposed, the sound waves play upon this physiological piano." I think, too, it must have been the keyboard of the piano that prevented a writer in 1892 (*Proc. Am. Acad.* 27, p. 222) from seeing any reason in the nature of things why, "under the known action of the separate vibrating parts of the ear," a single sound-wave or even a small portion of a simple sound-wave should not produce the sensation of definite pitch. To be sure, if "the invisible piano player" in the cochlea has suitable chunks of sound-wave at his fingers' ends, and knows how to hit the proper digitals with them, why should he not give a very pleasing rendering of *Die Lorelei*?

90. It soon proved that Thomas Young's advice, to hear what comparative anatomy had to say, was good, and that the efforts of Helmholtz had been misdirected, when C. Hasse dedicated his brief Latin dissertation to his Kiel professors, Henle and Hensen, appending the blunt theses (1) that the organ of Corti is lacking in birds, and (2) that the organ of Corti, for the purpose assigned to it, is not much good—*non multum valet* (1867). In the third edition of his book (1870) Helmholtz therefore abandoned the arches or fibres of Corti, and seems at the same time to have abandoned the idea of providing a rational or coherent anatomical basis for the resonance

theory of hearing. His corrections or modifications were carried out in a perfunctory manner; the reader is invited, in fact, in one obscure and important respect, to make his own corrections (p. 230; 1885, p. 147). It is not exact to say, as has been many times repeated, that Helmholtz now adopted Hensen's view, who supposed, following Corti, p. 163, that segments of the basilar membrane are set in transverse vibration by tones corresponding to their width (*Zts. f. wiss. Zool.* XIII., 1863, p. 507). The truth is that Helmholtz, without acknowledging his further debt to the Corsican, himself went back to Corti, as the following extract shows: "Quant à la zone pectinée nous ferons observer pour le moment que sa structure paraît partager en même temps les propriétés physiques des membranes tendues, et celles d'une couche de cordes tenues parallèlement et très-rapprochées les unes des autres. En effet on peut comparer les grossissements cylindriques de cette zone à des cordes de piano très rapprochées les unes des autres et soudées ensemble" (p. 164). Corti then refers to the similar suggestion made by Hannover (1844), but will not concede that there are *fibres* in the pectinate zone. Corti adds that he set down these reflexions in order to give rein to the imagination after exercising his patience. "Ce n'est naturellement qu'au moyen de l'exacte application des lois de l'acoustique, qu'on pourra expliquer la fonction de cette partie merveilleuse de l'organe de l'ouïe; et c'est ce que j'espère d'entreprendre bientôt" (1851, p. 165). It was to this point, then, that Helmholtz returned, and here he brought his mathematics to bear. Bowman had pointed out (1847) and Hensen repeated (1863) that the middle or pectinate zone "tore in the direction of its lines," and Helmholtz by assuming that the longitudinal tension in the basilar membrane is

"infinitesimally* small in comparison with" the transverse tension—the tension of a string 1/50 of an inch long and of microscopical thickness having a periodic time of 1/33 of a second must of course, by Brook Taylor's formula (1715), be something enormous!—was able to introduce zero and infinity* together with some thirty alphabetical symbols into his Appendix XI., a mathematical demonstration that the membrane behaves like a system of stretched strings, each acting independently of the others (M'Kendrick, 1899, p. 154),—a work of supererogation for one who can grant the assumption from which it sets out, and to one who cannot do this, proving nothing. The reason for rejecting the outer pillars of Corti's arches as a system of tuned strings, because "their lateral connections are strong enough to make them hang together in masses like a membrane" (Ellis, 1885, p. 141)—*like* a membrane—surely applies with equal or greater force to a real membrane which must be torn across in order to show that its middle portion is made up of thirty, twenty-four, or sixteen thousand fibres; and when we learn from Professor Keith (1918, p. 173) that Hensen's measurements as accepted by Helmholtz were wrong, and that the basilar membrane, instead of increasing in width in the proportion of 1 to 12 is "less than three times as wide at its termination in the apex as at its beginning in the base of the cochlea," the idea that it contains thousands of resonators accurately graded to respond to differences of one vibration per second or even less, becomes perhaps a little extravagant.

* This is a stale device. In the 3rd edition of Maclaurin, *Newton's Philosophical Discoveries*, 1775, p. XVII., I find the following:—"Nor can it be denied, that the terms *infinite* and *infinitesimal* were become much too familiar to mathematicians, and had been abused. . . . : At one time introducing and palliating real absurdities, and, at other times, giving those sciences an affected mysterious air which does not belong to them."

91. The counting and re-counting of cochlear fibres, if undertaken merely for the purpose of confirming or refuting Helmholtz, is a waste of time, for it is assumed that if a given tone have no fibre exactly corresponding to its pitch, the two nearest fibres, one on either side, will arrange a kind of compromise, and although each nerve fibre "hears in its own peculiar pitch," two neighbouring fibres between them may hear a tone of a pitch which is peculiar to neither. In fact, according to Ellis (1885, p. 147) "even if it should be found that many more than 4,200 degrees of pitch could be distinguished in the octave, it would not prejudice our assumption"; but this is a mistranslation,—it should be 4,200 in the *seven* octaves employed in music (*in der ganzen Scala*), or 600 in the octave. There would be less excuse for those who fail to comprehend how the comparison of two different intensities can produce a sensation of tone of another pitch, if Helmholtz did not seem to contradict himself, a page or two later, by stating that on the analogy of Thomas Young's hypothesis of three kinds of nerve fibres in the eye, he, Helmholtz, by assuming thousands of kinds of fibres in the auditory nerve, has reduced the *qualitative* difference of pitch to a "difference in the nerve receiving the sensation" (a sensation being sometimes apparently, and quaintly enough in this translation from the Double-Dutch, something objective;—"*evoking* the sensation," rather), while "for each individual fibre of the nerve there remains only the quantitative differences in the amount of excitement" (p. 148). It is evident that if one quantitative quality, amplitude-intensity, may on occasion be transmuted into another, frequency-pitch, and this equivalence be extended, the number of internal vibrators and of kinds of fibre in the auditory nerve may be reduced indefinitely; but then the theory would

provide only for an ear which is to a less or greater degree tone-deaf, and apt to confuse pitch with intensity. In testing a tone-deaf ear with two tuning-forks making various intervals up to a minor third, I found that either fork was likely to be declared the *higher* if its sound were sensibly the louder of two successive notes. Such confusion is common enough; there is proof of it in comparing French *parlez haut* with English *speak loud*, etc. But the mechanism of the peripheral organ must be at least as perfect as the well-authenticated observations which it allows the central organ to make. A nice musical ear does not confuse pitch with intensity when the experimental vibrations are maintained at a suitable amplitude, and the sounds no not "die away." Let us see how Professor Auerbach, on whom the mantle of Helmholtz has fallen, meets the objections of those who, unlike Professor M'Kendrick (*H. v. Helmholtz*, 1899, p. 156), hold that there is *not* a sufficient number of vibratile masses to satisfy this theory. Auerbach admits (1909, p. 226) that a quarter of a vibration per second suffices for the differentiation of two tones heard in succession in the most favourable circumstances, giving an extreme total of about 64,000 distinguishable pitches. On p. 668 Auerbach says that the numbers of fibres and of distinguishable tones are both of the order of magnitude 4,000 to 8,000; then contradicts himself by admitting that the number of tones (50,000) may be five to ten times as great as the number of fibres, and repeats what Helmholtz had said about two neighbouring fibres making good any deficiency. A favourite argument in favour of Helmholtz, as against *e.g.* Rutherford's "telephone-theory," is that he does not *relegate* the analysis of sounds to the central organ. But the Helmholtz theory evidently does relegate the finer discrimination of

pitch from the peripheral to the central organ. It places an elephant of thousands of vibrators upon a tortoise of thousands of different kinds of nerve fibre, and then breaks down. The theory is no good if it accounts merely for a poor or a moderately good ear: the mechanism in the cochlea must be at least as fine as the perceptions of the finest ear. A tone-deaf adult is demonstrably able, by giving attention to suitable exercises, to gain an improved sense of pitch, and may thereafter soon forget what has been learnt. No one would suggest that changes have taken place meanwhile in the structures of such a person's internal ear,—at least, I do not know of anybody calling in an invisible piano-tuner. The parrot, with only half a whorl to its cochlea, where there are 2½ whorls in the human ear, and without any fibres, arches, pillars, rods, poles or perches of Corti, is able to imitate speech and whistle musical notes; while the guinea-pig, with no less than four whorls and a magnificent organ of Corti, such that one might imagine its possessor capable of turning out a complete sonata before breakfast, is never heard to produce anything more beautiful than squeaks or grunts. No doubt we may say of the guinea-pigs, as Holder remarked about the birds that are able to articulate words—an accomplishment which the Piltdown man is said to have lacked—"we see that their souls are too narrow to use so great an Engine."

92. It seems extraordinary to speak of a difference of pitch as a *qualitative* difference. The German word *Tonhöhe* (tone-height) does not suggest or encourage such a view. I think it can be shown that we derive the musical notion of high or low pitch from the rise and fall of the larynx in the production of musical notes and intervals. A certain tone-deaf person (whose ears I'm afraid I have worried a good deal in recent years), never

having sung, hummed or whistled a set note, could not see any sense in calling one note high and another low. Since those who have a good sense of pitch, even if they have no knowledge of its physical meaning (frequency or period), appear to think of something in one dimension, I doubt the applicability of arguments drawn from visual sensations, which cannot be in less than two dimensions, to what is merely a relative sense of high or low, or, if there is a sense of absolute pitch, of higher or lower. The answer to a question *how much* (quantity) can generally be indicated along a straight line, or by a number; the answer to a question *what sort of* (quality), rarely so. Now the pitch of a tone, from the physical point of view, is perfectly defined by its vibration number. It seems more natural, therefore, to suppose that differences of pitch, being quantitative, may be conveyed by one kind of nerve than to postulate thousands of different kinds of nerve fibre to convey thousands (but not enough) of *qualities* of "tone-height," for it is only the tone-deaf person who inquires "What sort of high?" The musical ear is concerned with *to what extent* high. But here we come up against barbed wire. Frogs' legs and Müller's Law! *Verboten*!

93. It would appear frivolous to suggest that Joh. Müller achieved immortality by the epoch-making discovery that a man does not see with his nose or taste with his ears. That would be one way, but not the best way, of stating his law of specific energies. "The law amounts to this, that, *however excited*, each nerve of special sense gives rise to its own peculiar sensation" (Bayliss, *Principles of General Physiology*, 1915, p. 513). It is interesting to learn from Professor J. A. Thomson that "of this law Bunge says that it is 'the greatest and deepest truth ever thought out by the human intellect,' and Helmholtz with

equal hyperbole, compared it to the law of gravitation." It is well to have a sense of proportion. Helmholtz claims to have taken "a step similar to that taken in a wider field by J. Müller" (p. 148) by assuming differences in thousands of auditory nerve fibres. Professor Bayliss has pointed out (*Nature*, June 13, 1918, p. 295) that this law had already been formulated by Sir Charles Bell, though perhaps in not so complete a form; and Professor Keith is of opinion that the step which Helmholtz took is not in conformity to Müller's law, but at variance with it (*Nature*, Oct. 31, 1918, p. 165). The benefit conferred by a law which experts interpret in contrary senses is not obvious to the layman, on whose behalf it may be noted that in rehearsing the familiar comparison between nerves and wires conducting an electric current (1885, p. 149) Helmholtz does not mention the telephone, invented by Mr. Graham Bell, the son of Sweet's teacher, in 1876, the year before the last revised edition of *Tonempfindungen* was published. The passage is worth quoting. "Nerves have been often and not unsuitably compared to telegraph wires. Such a wire conducts one kind of electric current and no other; it may be stronger, it may be weaker, it may move in either direction; it has no other qualitative differences. Nevertheless, according to the different kinds of apparatus with which we provide its terminations, we can send telegraphic despatches, ring bells, explode mines. . . . So with the nerves." Since a telephone wire does undoubtedly convey "the qualitative differences of pitch and quality of tone," not perfectly perhaps, but practically so, then if the analogy of nerve and wire holds good, we may dismiss the specialized nerve-fibres of Helmholtz and of Holder as unnecessary, or we must imagine that telephone-wire is composed of thousands of specialized wirelets, which act as a kind of practical

Fourier's theorem! When Helmholtz first heard of the telephone, he remarked in a letter to Du Bois-Reymond, "The invention seems so self-evident, that I do not consider it necessary to advance a theory. Of course, I have for years gone to bed with Fourier's theorem in my head, and I have got up with it still there, so I must not judge others by myself." Is this utterance quoted by Professor M'Kendrick (1899, p. 167) for our admiration, or as a warning? It is safe to say that the convinced Helmholtzian might go to bed with the *Sensations of Tone* and Fourier's theorem on the brain to the end of his days, and yet fail to invent the telephone, unless the paradoxical idea occurred to him in some crazy dream.

94. It seems that we may not assume similarity in the conveyance of sound-messages in telephone wire and auditory nerve, for contrary assumptions are already in the field. Helmholtz decided that each nerve-fibre "hears in its own peculiar pitch," the vibratory character of sound terminating at the outer ends of the nerves along which the communication with the brain is established. It is generally, as Dr. William Holder says (1669), "easier to affirme, than to disprove," and apparently no means could be devised for testing this assertion until Lord Rayleigh, as a result of experiments on the capabilities of the ears in estimating the direction of sounds, suggested in 1907 that "on the contrary, the processes in the nerve must themselves be vibratory, not of course in the gross mechanical sense, but with preservation of the period and the characteristic of phase—a view advocated by Rutherford in opposition to Helmholtz as long ago as 1886" (quoted from Barton, *Text-book of Sound*, 1908, §615). But this conclusion is supposed to run counter to such physiological evidence as is available. "The work of Keith Lucas and his colleagues has shown that

the process set going in a nerve-fibre has a definite time-course and magnitude, whatever be the way in which it is produced," but " Professor Keith has pointed out that the work of Keith Lucas and Adrian on the 'all-or-nothing' character of the nerve-impulse was done on motor nerves only. . . . At the same time, we know of no such differences in the properties of motor and sensory nerves as to suggest a fundamental contrast of the kind required " (Bayliss in *Nature*, Oct. 17 and Dec. 5, 1918). Physiology, then, does not at the present time claim with Helmholtz (p. 149) that " we see that the mechanism of the process of irritation in the nerves of sense must be in every respect similar to that in the nerves of motion." It has no proof that it is so. It may be so. It is assumed to be so. But " why these nerve-impulses, alike in themselves, give rise to such widely different sensations "—as light, sound, taste, etc.—" when they arrive at their cerebral terminations is a matter beyond the scope of physiological analysis " (Bayliss, 1915, p. 513). A good answer to the question " What is Sound ? " was given by Lord Kelvin when suddenly clapping his hands together he told his audience, " There, that's sound." Since little, if anything, is known of the real nature of sound, and bearing in mind that the 1885 translation of Helmholtz's book does not mention the telephone-wire, which without " qualitative differences " conveys pitch and quality of tone quantitatively, the assumptions which Helmholtz made or adopted for the benefit of his theory cannot be permitted to stand as permanent barriers preventing further inquiry.

95. W. Rutherford thought of the telephone in connection with hearing in 1880, but did not publish anything on the subject till 1887 (*Journal of Anat. and Physiol.* XXI., p. 166; *On Tone-Sensation*, Edinburgh, 1898;

Brit. Med. Journal, II., 1898, p. 353), having no evidence of the possibility of sending a rapid succession of vibrations along a nerve. Ten instantaneous shocks of induced electricity in a second, sending ten impulses along a motor nerve of a frog, produced ten distinct contractions of the muscle, but 40 impulses a second gave "a single continuous contraction, because the several contractions are fused together." Apart from the suggestion of a possible analogy between this fusion and the fusion of rapid periodic shocks or taps, as from Robert Hooke's toothed wheel, into a note, I find it difficult to see what bearing this galvanizing and tetanizing of frogs' legs can have upon the perception of sound. Brain is not muscle; and nerve impulses are not electricity, though many arguments had to be brought forward before men of science gave up the notion that they were identical (D. Fraser Harris, *Nerves*, Home Univ. Lib., p. 43). Rutherford was not satisfied with such negative evidence, and inferred from the note of the honey-bee in flight that in the motor nerve of its wing there must be passing 460 vibrations in a second. He had no proof that more rapid vibrations could be transmitted. Yet if the note of the female gnat is produced in the same way—in my remembrance of the note it sounds in a much higher octave—there was no necessity to stop at 460. When a soprano voice reaches C in alt (c'''), we know that there is a kind of double-reed movement in the larynx repeated more than 1000 times in a second. But perhaps the following is a better example. I cannot by a voluntary effort make my teeth clap together ten times in a second, and I presume that if the attempt were made to make them chatter with the frequency of a low note by means of an alternating current of electricity passed into the nerves and muscles which open and close the mouth, the result

would be a sort of lock-jaw. But if with the teeth in loose contact, not clenched, I hum any note, the teeth tap out the same note, the alternating compressions and rarefactions of the air in the mouth moving the jaw synchronously through a minute angle. My top note is about 550. A soprano voice could probably make the teeth tap out a note of double the frequency without any risk of tetanizing maxillary muscles. Similarly, the wooden plug of a strongly vibrating 256 fork being pressed hard against the jaw, the teeth produce another note 256*. Forks of higher pitch, if of sufficient weight and amplitude of vibration, would no doubt be effective in the same way, by *synkinesis*. Thus the pitch of the note made by the teeth might perhaps be raised to the musical limit, say 4,000 per second, if not to the limit of definite pitch, or of audibility. (Are these limits the same for tones heard by bone conduction ?) I don't see why it should not, though it makes me feel uneasy to think of my teeth chattering at the rate of 16,000 or 40,000 times per second. But the numerical aspect of these things is very alarming. If I were suddenly to ask a boy who can whistle to set the air vibrating at the rate of 1,024 times per second, he might hesitate and make uncomplimentary reflexions; but if I sound a 512 tuning-fork and ask him to whistle that note, he will probably comply well enough with my former request, unless he makes the number *circa* 2,048. (I know nothing about the eye. I do not even know why we may not assume that colour, being a *spatial* phenomenon, is graded by the peripheral organ according to the wave-length rather than the frequency—hundreds of

* Under the combined action of the c′ fork and a falsetto hum the teeth may be made to mark *beats*, which, as the interval between fork and voice increases, unite in a " difference-tone," *i.e.* a beat-tone. This is an inexpensive pendant to the Barrett and Belas experiment with a water-jet.

billions per second—in *time*. With sound we may begin, if we wish, logically, like Sauveur, with a grandfather's clock, one tick per second, C_8 in T. Young's tablature, and work our way up. But to begin with a frequency of 400,000,000,000,000 or so per second would be a very queer start at any time.) In whistling there are no nerves or muscles actively concerned in the vibratory movements, "the adjustment of pitch (from about c'' to c^5) being effected mainly by varying the internal capacity," not by vibrations of the lips (Rayleigh, II., p. 224). But in other cases, as in the tapping note of the teeth, motor nerves are *passively* involved in the vibratory movement. Since Professor Barton includes Lord Rayleigh's conclusion of 1907, as above quoted (§94), in his text-book, I presume that he has repeated the experiment and confirmed the observation, for I conceive it to be the duty of the acoustician endowed with a keen sense of hearing to use his ears, to listen. There may be some flaw in my reasoning. The word *acoustician* may harbour the etymological fallacy. But as far as I can judge, we have at present the positive evidence of acoustics against the negative evidence (or the lack of evidence) of physiology that a sound-message with its "qualitative differences of pitch and quality of tone" may be conveyed by the auditory nerve to the appropriate nerve-cells quantitatively. I shall be convinced that this is not possible when I am permitted to read, from the pen of some enthusiastic admirer of the *Sensations of Tone*, the fascinating narrative of How Helmholtz Invented the Telephone.

96. The word *synkinesis* may be thought unnecessary, but I find it useful, because I cannot bring myself to speak of *sympathetic vibration* of the jaw keeping time with a hummed note or with the strong impulses carried through the stalk of a tuning-fork; while *resonance* would imply

a natural tone or resonance-tone in the system responding to the exciting force. I borrow the word from Arist. Quint., and use it, not with his meaning, but in the literal sense of *a moving with*, of a body which in vibrating moves passively with and in obedience to the force acting upon it, without manifesting any preference for a special frequency. The ideal diaphragm of the telephone, or phonograph, gramophone, etc., would act synkinetically, being aperiodic in relation to the periods of the vibrations transmitted, beginning and ending its vibratory motions accurately with those vibrations, without awaiting any summation of impulses before it can get into its swing—having no swing to get into—and without continuing to execute extra vibrations after the force which moves it has ceased to act. It would return to its position of rest, and there would be an end to the business. Helmholtz is not clear on *free* as opposed to *forced* vibrations. His *Mitschwingung*, which Ellis translates by *sympathetic vibration*, is sometimes ambiguous, and even misleading. For example, on p. 144 we read that "an elastic body set into sympathetic vibration by any tone, vibrates sympathetically in the pitch number of the exciting tone; but as soon as the exciting tone ceases, it goes on sounding in the pitch number of its own proper tone. This fact, which is derived from theory, may be perfectly verified on tuning-forks by means of the vibration microscope." A microscope of extra power would be needed to verify the first part of the statement in the following case. Of two c″ tuning-forks if one is flattened with sealing-wax, struck, and brought stalk to stalk with the other, so that this also vibrates, beats are audible, and if the beats are rapid enough, the beating note is heard to rise in pitch as the driving force flags. With the rôles reversed the beating note may be heard to fall slightly. *Ultimately*,

no doubt, and *if*—, there would be nothing but the forced vibration, which could hardly be called sympathetic except by a Prussian. If the two strings of our Greek author's kithara had not been quite homophonous, he might have heard similar beats.

97. An ordinary c″ fork being struck may be audible 20 seconds later, and will by that time have executed about 11,000 free vibrations; but a rap under the chin will bring the teeth together with one jolt or jar, and the jaw and teeth regarded as a vibrating system appear to have no proper or natural tone to be elicited in this way. I suppose there is no elastic body exemplifying absolute synkinesis, but the free air seems to be taken as practically synkinetic, and I think there would have been a better chance for the deaf (in some cases) and the tone-deaf if the internal ear had been studied with the more natural hypothesis of synkinesis (with limits of frequency and amplitude) as a starting-point instead of the mystical notion of resonance or sympathetic vibration. The idea of Aristides Quintilianus (wherever he may have found it) is well enough for poetry, where it is probably widespread. Something of the kind is to be traced in the Latin hymn to St. John the Baptist, in which Guido of Arezzo early in the 11th century found names for notes of the diatonic scale, *Ut* queant laxis *re*sonare fibris *Mi*ra gestorum *fa*muli tuorum, etc. And beautiful indeed is the verse of the Koran that inspired Edgar Allen Poe, "And the angel Israfel, whose heart-strings are a lute, and who has the sweetest voice of all God's creatures." But the prose of facts would deal harshly with the fanciful theorist who should postulate resonant fibres in the cockles of the heart*—which if they can be warmed must have

* The N.E.D. derivation of this phrase is bookish, from *Tractatus de Corde*, 1669, by Dr. Richard Lower, the Oxford physiologist who assisted Dr. Thomas Willis in his researches, p. 25: "Fibræ quidem . . . spirali suo ambitu helicem sive cochleam satis apte referunt."

material existence—and comparative anatomy is more than sceptical of resonators in the cochlea of the ear. I believe that Sir Thomas Wrightson's view of the mechanism of the internal ear, that the movements therein are *dead beat* in their character, and that of his collaborator Professor Arthur Keith, that there are no anatomical structures in the cochlea which can serve as resonators, are correct, and that in accordance with this view the facts of audition, some of which are unintelligible on the resonance theory as elaborated by Helmholtz, are or will be susceptible of explanation. To take no further instance, just here, than the noises and notes produced by the tapping of the lower against the upper teeth—experiments which anybody can perform without spending a penny on apparatus. We hear a single tap, and feel it too if it is forcible enough. When the teeth chatter, as with cold, we hear the separate taps, the first as well as the second and the rest. When the teeth make a note we hear it begin and end at the same instant as the voice, and even if we do not hear any taps in the note, we know that the note consists of rapid periodic taps. (Such a note, inaudible except to the performer, whose mouth is closed, is heard mainly by bone conduction, and therefore better if the ears are stopped. The sound of a tuning-fork placed on one's head is made louder by stopping the ears.) We hear the taps not as taps but as a musical note because the first is succeeded by the second, the second by the third, etc., too rapidly for them to be perceived separately. Is it credible that the first tap of such a series, which would be audible as a tap if it were isolated, is not audible at all when it is the first of a series? If the first fails to make an impression, does the second succeed in doing so, or the third? It is not credible. On the contrary, it is plain that the first tap of such a

series marks the beginning of the note. The second introduces period—we can hardly expect to have a series of *one*—and if we could take two and no more from such a rapid series, the experiments of Herroun and Yeo (1891, *Proc. Roy. Soc.*, L., p. 318) and others prove that the two taps would be heard, not as two separate taps, but as a sensation of tone without a very definite sensation of pitch, perfect definition of pitch requiring a somewhat longer series. Now this fact alone has been taken by some (as C. H. Hurst, *Trans. Liverp. Biol. Soc.*, 1895, p. 321) as disproving all resonance theories of hearing; but to furnish a disproof is not so simple a matter. As long as the evidence offered by Helmholtz is accepted, that after the vibrations of a note have ceased *outside* the ear, that note remains audible for some such period of time as that occupied by 9.5 vibrations of the note, so long will resonance theories of hearing continue to find credence. That so few vibrations may suffice to define pitch certainly shows that it is unnecessary and even unreasonable to assume different receptors in the internal ear for notes and noises, but that position was abandoned in H[4], 1877. According to the revised theory a single tap or a single vibration would affect all the resonators, the whole basilar membrane; the second and subsequent vibrations of a tone, while acting each of them on the membrane as a whole in the same way as the first, would affect more and more powerfully the resonator, whether a single fibre, a group of fibres, or a strip of membrane, having the same periodic time as the given tone, so that a certain summation of impulses is required for the resonator to get into its swing and call up a sensation of tone of full strength; and the more accurate the tuning of each resonator, *i.e.* the stronger its preference for a certain periodicity, the greater the number of periodic

impulses required to produce the full effect. It seems to me rather useless for science teachers to relate shocking examples of bridges collapsing like the walls of Jericho with the cumulative swing of resonance, and to repeat the apologue of the Boy and the Pea-Shooter and the Railway Bridge with the " ifs " left out, if a great authority like Helmholtz may play fast and loose with physical determinations. While it may be merely surprising that " even with so few as two vibrations a fair estimate of the pitch can be formed " (Barton, 1908, §573), what the resonance theory does not explain is that we can hear a note *beginning* at full strength and diminishing in intensity, as well as we can hear a note beginning soft and becoming loud, or another keeping at the same level of intensity. This fact is admitted by Helmholtz, p. 209, " On the piano the note is powerful only at the moment when it is struck, and rapidly decreases in strength." It surely requires some explanation.

98. And further, since in the seven octaves employed in music the period varies as sixty-four to one, a low note would demand a longer *time* than a high note to attain to its full intensity in the hearing if there is any summation of impulses. The perception of a bass note on the piano would therefore lag behind that of a note in the treble if the two are struck simultaneously; in fact, a high note might be well on its way to extinction before the low note is perceptible at its full strength. But a pianist strikes all the notes of a chord at the same instant, unless he desires an arpeggio effect. I am aware that there are some who handle the instrument as though it were a sort of canoe, striking their chords with a paddling action, first with the left hand and then with the right, as if compromising with the resonance theory, which would require the Compleat Pianist to give precedence to the

little finger of his left hand; but this faulty technique is apparent to some ears at least. We must remember that two sounds must synchronize to within 1/100 of a second to be perceived by the ear as simultaneous; and a single vibration of any bass note below G takes longer than that. Here it might be argued, from the relativity of sense-impressions in time, that the pianist, being guided by his ear, which is quicker than the eye or the touch of his finger-tips, does in fact strike the bass notes first in a chord; but the argument would be very thin, and to forestall it one may point to the pianola or the mechanical reed-organ, in which the notes of a chord are timed to begin simultaneously, in a straight line at right angles to the direction of the roll. Phoneticians and students of living languages have to study the beginnings and endings of sounds, and while the evidence from beginnings here adduced points decidedly to a synkinetic or dead-beat mechanism in the ear, the evidence from endings which will be brought forward presently will, to them at least, be conclusive, I think, against any and every resonance theory of hearing.

99. The action of the sound-board of the piano is interesting. The less the sound-board has of sympathy or predilection for any definite note, the better. "If the coefficient of damping were small, any notes of the pianoforte that lay near the natural tones of the sound-board in pitch would be unduly loud." It follows the motion of any string of the piano. "This is not a case of resonance, as the same sound-board has to serve for all the notes of the instrument" (Capstick, *Sound*, 1913, §172.) Possibly the floor-board mentioned by Professor Bayliss (*Nature*, Dec. 5, 1918) as felt to vibrate to "a particular note, especially to the powerful one of the trombone or drum" may have acted somewhat in this way, the amplitude of its response to less powerful notes and those

of higher pitch being too small to be felt in the same manner. If the intensity of a tone varies as the square root of the amplitude; and further, as Kœnig estimated in 1876, defending his beat-tones against a misapprehension which Tyndall had repeated from Helmholtz, the vibrations of a tuning-fork must have four times the amplitude of those of another fork an octave higher for the two sounds to have the same "intensité physiologique" (1882, p. 146), we can understand why the vibrations of loud low notes may be felt by placing a finger on certain parts of a gramophone, without appealing to *resonance*. I have recently observed in a concert-room, a string orchestra performing, that at a distance of some thirty feet the tremor of any loud note from the bass viols could be felt passing up through the chair on which one sat, and that shifting one's ground made no difference. But this fellow in the cellarage was not a case of resonance

100. I cannot agree that it is unreasonable to pursue exact knowledge in this direction any more than in another. Numerical data have this advantage, that they may be checked. It was on account of the supposed exact application of the laws of acoustics that the vague conjecture of Corti as to the vibratory function of the basilar membrane appeared to have materialized in the hands of Helmholtz into a solid structure. And as nobody would go a-shrimping with a salmon net, so it is impossible, to my mind, for the *ear* to judge whether a vibratory movement in response to a sound is "practically dead-beat" unless its own mechanism is, to an equal or greater degree, of the same kind. But as that would mean that the basilar membrane is practically aperiodic, the resonance theory would thus become a displacement theory, or else remain at best " with some limitations, not wholly improbable," as in 1807. So to proceed.

101. The observation dates, I believe, from Newton (reference wanting) that high notes may be perceived in quicker succession than low notes. It is not hard to find reasons for this; not so hard as to find whether Newton gave any. There can be no note without periodicity, and until the second vibration of a note follows the first there can be no period. The period of a low note being longer than that of a high note—as sixty-four to one in the seven octaves of music—and good definition of the pitch of a note requiring more than two vibrations, it follows that a shake, or alternation of notes, must be slower in the bass than it may be in the treble for any one note to be distinctly heard. The boundary between any two notes of a shake in the bass would be further obscured if the resonance of the instrument employed prolong the sound of the former note by any number of audible vibrations. Helmholtz tried his shakes on the 'cello and the harmonium, and assumed that "each separate tone is completely cut off with perfect certainty at the moment the valve falls on the air passage," or the fingers change the length of the 'cello strings, forgetting or not having learnt that the sound given out by the reed or string would be very faint but for the spread of the vibrations in the body of the instrument. Not taking these things into consideration he was "forced to conclude that the difficulty lies in the ear itself. We have, then, a plain indication that the vibrating parts of the ear are not damped with sufficient force and rapidity to allow of successfully effecting such a rapid alternation of tones" (p. 143). From this incautious conclusion he drew the further inference, borne out by "observations on beats subsequently detailed" that since below A (110) a shake or trill of ten notes in a second begins to be indistinct, the amount of damping power in the ear corre-

sponds to the third degree of a certain tabulation; in other words, that " the intensity of sympathetic vibration with a Semitone difference of pitch is only 1/10 of what it is for a complete unison. Of course there can be no question of exact determinations, but it is important to be able to form at least an approximate conception of the influence of damping on the sympathetic vibration of the ear, as it has great significance in the relations of consonance " (p. 144).

102. There is a relation between the magnitude of the interval over which an elastic body will respond to an exciting tone and the number of free vibrations which it can execute. " Thus if the interval between the natural and the forced vibration required to reduce the resonance to 1/10 of the maximum be a semitone, this implies that after 9.5 free vibrations the intensity would be reduced to 1/10 of its original value, and so on for other intervals. From a consideration of the effect of trills in music, Helmholtz concludes that the case of the ear corresponds somewhat to that above specified " (Rayleigh, II., p. 449). Professor Bayliss (*Nature*, Dec. 5, 1918) now appears to require that in the case of the ear this relationship should no longer exist, and that the basilar membrane should have a coefficient of damping both small and large at the same time; or, to be more precise, that it should be small during the perception of a definite note, so that " the amplitude may be sufficiently great to be effective only in immediate proximity to the maximum," which would upset the Helmholtz relations of consonance and his observations on beats; while at the end of a note the coefficient of damping is to be so very large that the vibration of the strip of membrane may be " practically dead-beat "—as though there actually were pianoforte dampers in the cochlea to be applied at will.

103. It is in my opinion no exaggeration to say that the estimate of the duration of resonance which Helmholtz formed from his consideration of the effect of shakes is "the very key-stone" of his theory of audition (*Nature*, Nov. 7, 1918), though perhaps it would have been better to write, for the enlightenment of those who believe that the whole question begins and ends with Helmholtz, "the very key-stone of the resonance theory of audition as elaborated by Helmholtz," or to have quoted from the *Theory of Sound*, if the book had been at hand: "The magnitude of the interval, over which a single internal vibrator will respond sensibly, is an element of considerable importance in the theory" (II., p. 449). The estimate, or guess, of 9.5 free vibrations, corresponding to the interval of a semitone on either side of a maximum, which fits in so well with the observations on consonance on the one side and the observations on beats on the other, so that the whole fabric has the appearance of stability and self-support, is, in point of fact, the only item of direct evidence alleged in the whole book that there are tuned resonators in the cochlea. The rest is superstition, speculation, and combination. The beginning of the next paragraph on p. 143, in which Ellis's italics represent spaced type in the original, shows how much importance was attached to the inference from shakes: "Nay more, this fact further proves that *there must be different parts ot the ear which are set in vibration by tones of different pitch and which receive the sensation of these tones.*"

104. Now if there were no possibility of approximately more closely to reality in this matter it would be idle to question the findings which have passed unchallenged for half a century, and the present writer would not have ventured into the correspondence columns of *Nature*

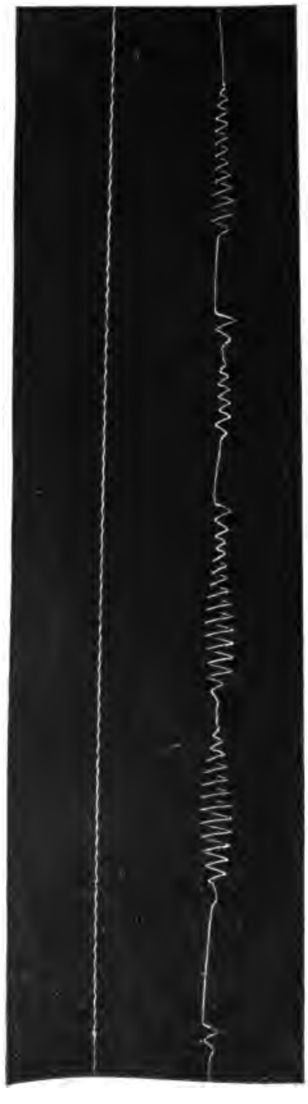
merely to experience the sensations *d'un chien dans un jeu de quilles*. But it is clear that if we wish to give the ear a fair chance it will be better to take the simple case of a single note, and not to judge from the impression left by a trill or shake, which is at best but shaky, owing to the confusion, under certain conditions, of pitch with intensity. We speak of a trilled *r* where there is evidently, on reflexion, no alternation of pitch, but an alternation of intensity. So, too, a tracing from a gramophone record of what was supposed to be a proper trill executed by an Italian singer, proved when magnified and measured to be no trill, but a tremolo, the wave-length remaining constant while the amplitude alternated (Scripture, *Speech Curves*, 1906, p. 36). If we consider a single note, at once the question arises, how can we hear a loud note end staccato if there are resonators in the ear which continue to vibrate audibly with diminishing amplitude for an appreciable time after the " sound " has ceased at its source ?

105. It seems to me that Helmholtz, having learnt much from Thomas Young, might have learnt more. How accurately can the ear judge of the termination of a sound ? Here at all times a measurement independent of the ear would have been in place, and such a measurement might have been attempted by an investigator having access to apparatus at any time since the incomparable Dr. Young, when dealing with the subject of chronometry, conceived and carried out the idea of rendering Father Time capable of becoming autobiographical, *i.e.* of registering course and recourse in periods as brief as one-thousandth of a second, in the shape of a rotating cylinder, the parent of all myographs, phonautographs, phonographs, kymographs, etc.* Certainly the

* Perhaps the idea was suggested by the " rundle, cover'd with Paper, upon which the Clock moved a black-lead-Pensil," employed

objective duration of the mute or silence in such a word as *little* might have been measured, after it had been pointed out by Sir Charles Wheatstone in 1831 that the musical inflections of the voice may be perfectly transmitted by a conductor connected with some part of the neck contiguous with the larynx (*Scientific Papers*, 1879, p. 62). In Dr. Scripture's Cock Robin plate, from a gramophone record, the straight line for *t* in the word *little* measures 47mm., or, one mm. being equal to 0.0016 sec., 0.075 sec. That is to say, the interval between the two sounds of the voice on the record is a shorter time than the ear would need, according to Helmholtz, to reduce the sound of the first syllable to 1/10 of its intensity if the voice were pitched no higher than about *c*. But in *little* it is not a question of reduction to 1/10. The sound is reduced to *silence*, and the silence itself occupies a perceptible portion of the 0.075 sec. (No matter how rapidly one may speak or whisper the word *little*, the sound of the second syllable is heard to begin *from silence*). Since, however, in speech the pitch of the voice is constantly changing, it was necessary, in order to meet Helmholtz's contention squarely, to measure sounds and silences in words intoned on a definite note, preferably below the critical pitch of A(110). This was attempted by means of a kymograph in the phonetics laboratory at University College, London, and a measured mouth-tracing of the word *utter* from a

by Dr. Christopher Wren in his recording weather-cock and recording thermometer, as described by Sprat, *Hist. Roy. Soc.*, 1667, p. 312 (quoted in *Dic. Nat. Biog.*), and in the son's *Parentalia*, pp. 208, 244. But while Wren was content with an *adagio*, Young wanted a *prestissimo*. "Toute la chronographie est contenue en germe dans cette invention de Thomas Young," writes Marey in his *Méthode graphique*, 1885, p. 110; but I find no mention of Sir Christopher in this book, which on pp. 108, 113 dates the origin of such self-registering apparatus about 1734.



phrase intoned rapidly by Mr. Stephen Jones, with the voice pitched at about 100, was reproduced in *Nature*, May 16, 1918, p. 204.* Another tracing, of the whole phrase, made on the same occasion, is here given. The third straight line from the top of the page represents the time during which the tongue forms the *t*-closure in the word *utter*. The length of this line may be translated from space into time, with the help of a pair of compasses, by measuring it along the contemporaneous tracing left by a tuning-fork of pitch 100, in which each wave, from crest to crest, represents 1/100 second. It will be seen to be just about 1/20 second long, while 9.5 vibrations, measured back from the point where the tongue comes away from the palate, would take us more than half way through the preceding vowel in addition to the mute.

106. If, then, our ears were constructed on Helmholtz's plan, we could not distinguish *utter* from *udder*. We could not perceive that short, sharp shock, the effect of *p, t, k* in any such words as *apple, bottle, pocket*. We could not have imitative words like *tap, rat-tat, tick-tack*. For what we cannot hear we cannot by the ordinary process learn to pronounce. The English language, like most other languages, would be an impossibility. Helmholtz was *not* "able to form an approximate conception of the influence of damping on the sympathetic vibration of the ear." His estimate does *not* stand in need merely of some slight correction. It is ludicrously false. There is no reason to think that the ear deals with notes otherwise than with noises, since it is impossible to draw a clear boundary line between the two classes of sounds; and

* (July 1919). Another specimen of the same had previously been submitted to the late Lord Rayleigh, who very kindly replied (May 27, 1917), "Your argument to show that Helmholtz's estimate of the duration of resonance needs correction seems cogent." I had not at that time reached my present conclusion.

1/100 second is probably as effective with the one class as with the other. Helmholtz should have abandoned the hypothesis of sympathetic vibration in his 4th edition, 1877, when the otoliths were relieved of the auditory function previously assigned to them. But some of his followers in this country have been quite content with the third edition, of 1870, translated by Ellis in 1875. Thus Professor M'Kendrick in his eulogistic life of Helmholtz (1899) does not mention in his bibliography the 1877 edition of *Tonempfindungen* or its translation by Ellis in 1885, and both there (p. 164) and in Schäfer's *Physiology* (1900, p. 1218) gives the summary of Helmholtz's conclusions as to vowel qualities of tone from the 3rd edition, without revealing any knowledge of the fact that in the 4th edition a remarkably economical revision had taken place, and "not . . . but . . . " had become "not solely . . . but preponderantly . . . "—something like changing *minus* into *plus*.

107. The familiar symbols, *p*, *t*, *k* are apt to mislead. The definition given by Dr. John Wallis in 1653 is clear, however: "*p*, *t*, *k* . . . absolutè Mutæ sunt; nec ullum per se sonum edunt, sed solummodo sonum (sive præcedentem, sive subsequentem) modificant." There must be a passage of breath or voice either before or after, or both before and after, for the signs *p*, *t*, *k* to have any power of sound or phonetic value. They may be compared to current coins, not blank discs of metal, but stamped both sides, heads and tails, obverse and reverse, occlusive and plosive. Helmholtz, whom Ellis with all his foot-notes could not turn into a phonetician, divided vowels and consonants, very roughly, into his two classes, notes and noises. To him the consonant letters, except *m*, *n*, and the *ng* in *sing*, represented noises which have no constant pitch (p. 117). Can a silence be a noise?

It is a question for the psychologist. Apparently an objective silence can be a subjective noise. A sudden silence may have an effect similar to that of a noise. There are plenty of cases on record of people waking up when their bedroom clock stopped. An experiment may be tried on the gramophone by suddenly lifting the needle from the disc in the middle of a loud note. Thus a tenor, checked in the third syllable of "Onaway," seems to be trying to sing "Onaway, awake" in one portmanteau-word, without the plosive part of the *k*. Articulations of this kind are noted by Sweet (*Primer*, §143) as of regular occurrence in some East Asiatic languages; and there is the same kind of thing in the American "Yep" variant of "Yes," where the lips remain closed, as in the determined or sulky "Nope" for "No," sometimes uttered by English children. The shock effect of such an aposiopesis appears to be most striking in what Sweet called the "Glasgow" substitute for occlusive *t* or *k* (more rarely for *p*), where the loud voice in a short accented vowel is cut off at its source in the larynx by an instantaneous closing of the glottis,—as, with inhaled voice, in a hiccup. Is this a noise? If we have time to think it over, it is not a noise, but a silence suddenly supervening on a sound. This becomes evident if a long vowel intoned diminuendo be ended by the same action of closing the glottis—an action which, in itself, makes no sound whatever.

108. We are interested here in the occlusive aspect of the mutes, and the simplest case for examination will be the glottal occlusive, as in *bo'le, ma'er* (for *bottle, matter*). We know that in a full-quality chest note the glottal lips open and close completely in the period of the note sung, the closed phase being longer than the open phase; and that they emit *e.g.* for the note 128, 128 puffs of breath

per second at thoracic pressure, thus producing whatever sung vowel the supra-glottal cavities may be shaped for. This double-reed action has been shown on the kinematograph, the movements having been slowed down to suit the eye by means of the stroboscope. It is possible to let the pitch of the voice sink down below the limit of chest register, so that the glottal puffs are too slow or too irregular to cause a musical note, though not too slow for the secondary pulsations of Robert Willis to make vowels, since the pitch of the mouth-cavity shaped for English vowels rises (in my own case) from about *f″* (683). This kind of grating voice may frequently be observed in the speech of phlegmatic, sleepy, bored, insolent or sick children and adults. Slowing down still more, we may observe these puffs or pulses of sound in groups of three or four, or even succeed in getting a single one—Thomas Young knew this—just as the lips of the mouth may be made to emit one instantaneous puff of breath. (It would be well if the misleading designation "vocal cords," *chordæ vocales*, disputed ever since its introduction by Ferrein in 1741, could finally be scrapped).

109. Now when a stressed vowel sung on a chest-note ends staccato with the glottal occlusive, what happens? I have stated (§66; *Nature*, Nov. 7, 1918, p. 185) that the harmonic of the voice reinforced by the mouth cavity may in some instances be heard persisting for a very brief time after the voice itself thus suddenly ceases to be heard. I have no objective proof of this. I could not demonstrate it to a paper-acoustician who will neither listen nor think, and may therefore, likely enough, condemn my work as utterly unconvincing, unscientific, or what not. But it is what might be expected of such a resonator, if examined independently of the ear. "When the original cause of sound ceases, the resonator yields back the vibra-